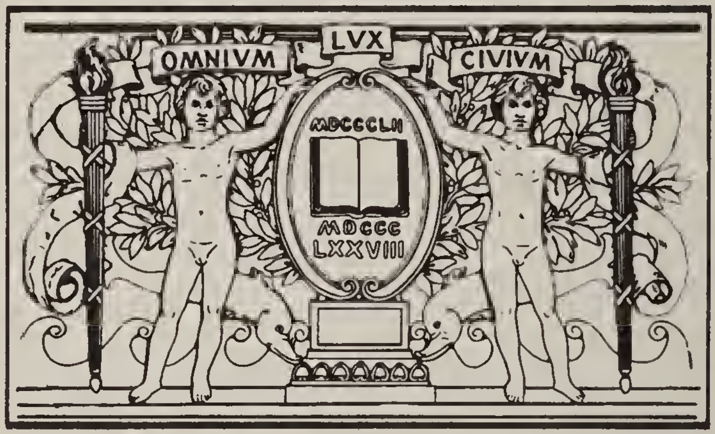

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from the Author

REPLY

TO

VARIOUS OBJECTIONS

WHICH HAVE BEEN BROUGHT AGAINST HIS

THEORY OF THE SECULAR ACCELERATION OF THE MOON'S MEAN MOTION.

By J. C. ADAMS, M.A.
LOWNDEAN PROFESSOR OF ASTRONOMY AND GEOMETRY,
AND FELLOW OF PEMBROKE COLLEGE, CAMBRIDGE.

Reprinted from the Monthly Notices of the Royal Astronomical Society (April 1860), *with additions and corrections by the Author.*

REPLY,

&c. &c.

If I have hitherto published no reply to the "Observations" of M. de Pontécoulant, contained in the Monthly Notices of July last, it is not because the task presented any difficulty, for the fallacies which pervade M. de Pontécoulant's communication were perfectly evident to me from the very first. I thought that any competent person who chose to look into my memoir "On the Secular Acceleration," and into these observations upon it, might be safely left to form his own judgment on the matter. Again, I had some hopes that M. de Pontécoulant might be led to see and acknowledge the errors into which he had fallen, and with that object in view I sent to him, on more than one occasion, through a friend, communications which appeared to me amply sufficient to expose the fallacies contained not only in his printed "Observations," but also in several private letters which he subsequently wrote upon the subject. I find, however, that M. de Pontécoulant, in a letter which he has lately caused to be circulated among the members of the French Institute, has ventured to ignore these communications of mine altogether, and to speak as if his observations had been admitted without dispute. Under these circumstances, as my further silence might be misconstrued, I beg leave to offer to the Society the following remarks.

In order to give a more complete view of the subject, however, and to obviate the necessity of my returning to it in a controversial manner, I shall not confine myself to the observations of M. de Pontécoulant, but shall likewise say a few words in reply to the objections of M. Plana and those of M. Hansen. I shall also take the opportunity of making some preliminary remarks which may tend to remove certain misapprehensions, which I have reason to believe exist in some minds with respect to the real nature of the matter in dispute.

First, then, I would call attention to the fact that the question is a purely mathematical one, with the decision of which observation has nothing whatever to do. It may be simply stated thus: if the excentricity of the earth's orbit be supposed to change at a given uniform rate and very slowly, what will be the corresponding rate of change, according to the theory of gravitation, in the mean motion of the moon? Now the solution of this question is effected by means of a purely algebraical process, the validity of each step of which admits of being placed beyond all possible doubt.

What conclusion must be drawn, then, supposing that ancient observations should show that the secular variation of the moon's mean motion is different from that which, according to theory, is due to the known change of the excentricity of the earth's orbit?

Why, simply this; that the mean motion of the moon is affected by some other cause or causes, besides the variation of excentricity which has been taken into account. This fact, if established, would be a most interesting one, and might put us on the traces of an important physical discovery. It is not difficult to imagine the existence of causes which may affect the mean motion of the moon, but whether it were so or not, any question respecting the validity of a mathematical process must be decided on mathematical grounds alone, quite independently of the agreement or disagreement of theory and observation.

In the case before us the mathematical question as stated above may be greatly simplified, without its ceasing to involve the point which is in dispute. The values of the secular acceleration given by M. Plana's theory and mine, differ in terms which are independent of the excentricity and inclination of the moon's orbit; consequently in deciding which of the theories is right, we may suppose the excentricity and inclination to vanish.

In the next place I would remark that the error which I attribute to M. Plana's theory on this point is not one of calculation which might require long and complicated numerical processes to be gone through for its correction, but that it is an error of principle, about which a mathematician ought not to have much difficulty in making up his mind. I am therefore inclined entirely to agree with M. de Pontécoulant's opinion, that the prolonged discussion of this subject would not be creditable to science, and indeed, considering the importance of the question, and the length of time which has passed since the publication of my memoir, I cannot but think it strange that any controversy respecting it should still exist at all.

Some persons appear to be under the impression that the contest lies between two values of the secular acceleration, that M. Delaunay and I agree in one value, and that MM. Plana, de Pontécoulant, and Hansen, agree in a larger value; but this is by no means the true state of the case. Between M. Delaunay's result and my own, indeed, there is a perfect agreement. He has carried the approximation much further than I have done, but all of the terms which I have calculated have been confirmed by him. Again, before publishing my memoir in 1853, I had obtained my result by two different methods, and I have since confirmed and extended it by means of a third. M. Delaunay arrived at his result by an independent method of his own, and he has lately found exactly the same result by following the method given by Poisson.

On the other hand, among our opponents there is far from being the same satisfactory agreement.

In his theory of the moon, M. Plana obtained one value of the secular acceleration. In 1856 he printed a paper in which he admitted that his theory was wrong on this point, and actually deduced my result from his own equations. Soon afterwards, however, M. Plana retracted his admission of the correctness of my result, and obtained a third result, differing both from his former one and from my own.

Again, M. de Pontécoulant, in the last communication which I received from him, gives two different values of the secular acceleration, one of which he has obtained by using the time, and the other by using the moon's longitude as the independent variable. Strange to say, however, he does not appear at all startled at obtaining two contradictory values, but seems fully inclined to defend both. Indeed, judging from the last paragraph of his letter in the *Monthly Notices*, he appears to have expected that the results of the two methods would differ from each other. One of the values which M. de Pontécoulant thus obtains agrees with that given in M. Plana's theory, as of course it must do, being found by means of the same principles. But he seems to be quite unaware that this value has been abandoned by M. Plana himself in his last paper above referred to, which is contained in the eighteenth volume of the Turin Memoirs.

M. Hansen's value of the secular acceleration is not given in an analytical form, like those of MM. Plana and de Pontécoulant, and therefore we can only compare the final numerical results. This comparison, which I shall presently give, shows that M. Hansen's value of the acceleration considerably exceeds either of those found by M. Plana.

Here then we find nothing to inspire confidence; certainly nothing like the cumulative testimony which there is in support of M. Delaunay's result and mine.

I may now be permitted to make some remarks on another point. In the introduction to my memoir of 1853, I gave some general reasoning to show that a change in the excentricity of the earth's orbit had a tendency to produce a change in the mean areal velocity of the moon, and that M. Plana was therefore wrong in assuming this velocity to be constant, as in his theory he does. Now this seems to have led some persons to imagine that my analysis in the following part of the memoir depended in some way or other on the validity of the general reasoning which had gone before, and therefore that my conclusions could not be regarded as established with mathematical strictness. But this is quite a mistaken view of the case. I make no assumption respecting the variability of the mean areal velocity. I prove mathematically that this velocity does vary by finding the amount of its variation, and the general reasoning given in the introduction is simply the translation, so to speak, of my analysis into ordinary language, in order to make the nature of my correction to M. Plana's theory, more generally intelligible. It may be remarked too that even if I had started with the assumption that the mean areal velocity was variable, no error could have been caused thereby, for if this velocity had been really constant I should have found its variation equal to zero. In mathematics the terms "constant" and "variable" are not looked upon as opposed to each other, but a constant is regarded as a particular case of a variable quantity.

It may be as well to guard against the idea that the extreme minuteness of the quantities which we have to deal with in this investigation, gives rise to any uncertainty in the result. The present rate of approach of the moon to the earth which accompanies the acceleration of its motion, is less than one inch per annum, but the theory can determine this minute quantity to within, say, a thousandth part of its true amount, just as easily and certainly as if the quantity to be found had been any number of times greater.

I will now proceed briefly to explain the principles which I employ in determining the secular acceleration, and to point out the errors which vitiate the several results of MM. Plana and de Pontécoulant which have been already referred to.

The principle of my method is simply this, viz., that the differential equations must be satisfied, and that quantities which really vary must be treated as variable in all the differentiations and integrations which occur throughout the investigation.

Now if e', the excentricity of the earth's orbit, be variable, the differentiation or integration of any term which involves e' in its coefficient will produce, in addition to the term which would result if e' were constant, another term involving $\frac{d\,e'}{d\,t}$ in its coefficient, supposing t to be the independent variable.

In consequence of the existence of these supplementary terms, the ordinary expressions for the moon's coordinates when substituted in the differential equations will not satisfy them, but will leave terms multiplied by $\frac{d\,e'}{d\,t}$ outstanding. In order to destroy these terms, it is necessary to add terms of the same form to the usual expressions for the moon's coordinates. The values of these new terms may, if we please, be easily found by the method of indeterminate coefficients, each of the coefficients being obtained by means of a simple equation.

If n, the moon's mean motion, be variable, the double differentiation of the moon's coordinates will produce in the differential equations, terms involving $\frac{dn}{dt}$ of the same form as those already mentioned which involve $\frac{de'}{dt}$.

Thus the same system of simultaneous simple equations that gives the values of the indeterminate coefficients, determines likewise the value of $\frac{dn}{dt}$, which is what we want to find.

If the moon's longitude ν be taken as the independent variable, we must proceed according to the same principles, but there is one additional circumstance to be attended to.

In the former case, since e' is supposed to vary uniformly with the time, $\frac{de'}{dt}$ is considered constant, or $\frac{d^2e'}{dt^2} = 0$. In the latter case the terms which are introduced by the consideration of the variability of e' will involve $\frac{de'}{d\nu}$ instead of $\frac{de'}{dt}$ as before; and since the moon's motion in longitude is not uniform, the value of $\frac{de'}{d\nu}$ cannot be considered constant, or $\frac{d^2e'}{d\nu^2}$ cannot be neglected. To take this into account we must substitute for $\frac{de'}{d\nu}$ its value $\frac{de'}{dt}\,\frac{dt}{d\nu}$, in which $\frac{dt}{d\nu}$ is a known function of ν, and then the remainder of the process will be exactly similar to that before described.

Let us now consider the method followed in M. Plana's theory, and also by M. de Pontécoulant.

In this method the terms above described involving $\frac{de'}{dt}$ are ignored, and consequently the differential equations as developed by these astronomers furnish no materials whatever for determining the value of $\frac{dn}{dt}$. Hence they are forced to supply the lack of data by means of an assumption, which is that one of the so-called constants introduced by integration is absolutely constant.

The value of any one of the constants so employed can be expressed in terms of n, e' and known quantities. If then this so-called constant were really so, we should be able by differentiating this relation to obtain $\frac{dn}{dt}$ in terms of $\frac{de'}{dt}$. But if on the other hand this supposed constant be really variable, we must take its variation into account, in order to obtain the true value of $\frac{dn}{dt}$ in terms of $\frac{de'}{dt}$.

In M. Plana's theory, in which ν is taken as the independent variable, the constant so employed is h^2, which is added to complete the integral $2\int r^2 \frac{dR}{d\nu}\,d\nu$, in the equation

$$r^4\left(\frac{d\nu}{dt}\right)^2 = h^2 + 2\int r^2 \frac{dR}{d\nu}\,d\nu,$$

in which $2\int r^2 \frac{dR}{d\nu}\, d\nu$ is supposed to consist of a series of cosines of multiples of ν.

The quantity $r^2 \frac{d\nu}{dt}$ is equal to twice the area described in a unit of time, or to twice the areal velocity, so that h^2 is the non-periodic part of the square of twice the areal velocity, the periodic part being supposed developed in cosines of multiples of ν.

In M. de Pontécoulant's theory, the constant h is introduced to complete the integral $\int \frac{dR}{d\nu}\, dt$ in the equation

$$r^2 \frac{d\nu}{dt} = h + \int \frac{dR}{d\nu}\, dt,$$

in which $\int \frac{dR}{d\nu}\, dt$ is supposed to consist of a series of cosines of multiples of t.

M. de Pontécoulant's h is not identical with M. Plana's h, but there is a simple relation between these quantities.

M. de Pontécoulant, however, does not employ the constant h in finding the value of the secular acceleration, but another constant $\frac{1}{a}$, which is introduced to complete the integral in the equation

$$\frac{1}{2} \frac{d^2 (r^2)}{dt^2} - \frac{1}{r} + \frac{1}{a} = 2\int d' R + r \frac{dR}{dr},$$

all the periodic terms of which are supposed to consist of cosines of multiples of t.

If we neglect the excentricity and inclination of the moon's orbit, and also omit all powers of m above the fourth, the relations between these several constants and the mean motion n will be expressed as follows:

$$\mathrm{h} = n^{-\frac{1}{3}} \left\{ 1 - \frac{1}{3} m^2 + \frac{719}{576} m^4 + e'^2 \left[-\frac{1}{2} m^2 + \frac{2635}{384} m^4 \right] \right\},$$

$$h = n^{-\frac{1}{3}} \left\{ 1 - \frac{1}{3} m^2 + \frac{11}{144} m^4 + e'^2 \left[-\frac{1}{2} m^2 - \frac{185}{96} m^4 \right] \right\},$$

$$\frac{1}{a} = n^{\frac{2}{3}} \left\{ 1 + \frac{2}{3} m^2 - \frac{1253}{288} m^4 + e'^2 \left[\quad m^2 - \frac{5593}{192} m^4 \right] \right\},$$

the sum of the masses of the earth and moon being supposed to be unity.

From these relations we find by differentiation

$$\frac{d\,n}{n\,d\,t} = -\,3\,\frac{d\,\mathrm{h}}{\mathrm{h}\,d\,t} + \frac{d\,(e'^2)}{d\,t}\left\{-\,\frac{3}{2}\,m^2 + \frac{2187}{128}\,m^4\right\},$$

$$\frac{d\,n}{n\,d\,t} = -\,3\,\frac{d\,h}{h\,d\,t} + \frac{d\,(e'^2)}{d\,t}\left\{-\,\frac{3}{2}\,m^2 - \frac{297}{32}\,m^4\right\},$$

$$\frac{d\,n}{n\,d\,t} = -\,\frac{3}{2}\,\frac{d\,a}{a\,d\,t} + \frac{d\,(e'^2)}{d\,t}\left\{-\,\frac{3}{2}\,m^2 + \frac{5337}{128}\,m^4\right\},$$

having taken care to observe that, since $m = \frac{n'}{n}$ and n' is constant, we have $\frac{d\,m}{m\,d\,t} = -\,\frac{d\,n}{n\,d\,t}$.

If $\frac{d\,\mathrm{h}}{\mathrm{h}\,d\,t}$ be neglected in the first of these expressions, we obtain the value of $\frac{d\,n}{n\,d\,t}$ found in M. Plana's theory, and one of those found by M. de Pontécoulant. If $\frac{d\,h}{h\,d\,t}$ be neglected in the second, the resulting value of $\frac{d\,n}{n\,d\,t}$ is what would have been found by M. de Pontécoulant, if he had taken his own h to be constant instead of M. Plana's h.

If in the third expression $\frac{d\,a}{a\,d\,t}$ be neglected, we obtain the value of $\frac{d\,n}{n\,d\,t}$, which M. de Pontécoulant communicated to me as the result which he had found by using t as the independent variable.

It is obvious that these several values of $\frac{d\,n}{n\,d\,t}$ contradict each other, and the reason is that the quantities h, h, and a are really variable, and that therefore $\frac{d\,\mathrm{h}}{\mathrm{h}\,d\,t}$, $\frac{d\,h}{h\,d\,t}$, and $\frac{d\,a}{a\,d\,t}$ have been wrongly neglected. In order to find the true value of $\frac{d\,n}{n\,d\,t}$ we must therefore determine the values of these last-mentioned differential coefficients, and substitute them in the several expressions for $\frac{d\,n}{n\,d\,t}$ given above.

Now the supplementary terms involving $\frac{d\,e'}{d\,t}$ which I have shown to exist in the expressions for the moon's coordinates, will introduce into the integral

$$2\int r^2\,\frac{d\,\mathrm{R}}{d\,\nu}\,d\,\nu,$$

besides periodic terms, a non-periodic one of the form

$$\int \mathrm{H}\,\frac{d\,(e'^2)}{d\,t}\,d\,t, \quad \text{or } \mathrm{H}\,e'^2,$$

consequently, since in the equation

$$r^4\left(\frac{d\,v}{d\,t}\right)^2 = \text{h}^2 + 2\int r^2\frac{d\,\text{R}}{d\,v}\,d\,v,$$

M. Plana considers h^2 to denote the whole of the non-periodic part of $r^4\left(\frac{d\,v}{d\,t}\right)^2$, h^2 must consist of an absolutely constant part together with the variable quantity $\text{H}\,e'^2$ just mentioned

$$\text{and} \quad \therefore \quad \frac{d\,(\text{h}^2)}{d\,t} \text{ must be equal to H } \frac{d\,(e'^2)}{d\,t}.$$

Similarly $\frac{d\,h}{d\,t}$ may be found by determining the non-periodic term which is in the same way introduced into the integral

$$\int\frac{d\,\text{R}}{d\,v}\,d\,t$$

in the equation

$$r^2\frac{d\,v}{d\,t} = h + \int\frac{d\,\text{R}}{d\,v}\,d\,t\,;$$

and $\frac{d\left(\frac{1}{a}\right)}{d\,t}$ may be similarly found by means of the non-periodic terms introduced into the integral $\int d'\text{R}$, in the equation

$$\frac{1}{2}\frac{d^2\,(r^2)}{d\,t^2} - \frac{1}{r} + \frac{1}{a} = 2\int d'\,\text{R} + r\,\frac{d\,\text{R}}{d\,r}.$$

When all this has been done, and the proper substitutions made, the three expressions for $\frac{d\,n}{n\,d\,t}$ are found to agree in giving

$$\frac{d\,n}{n\,d\,t} = \frac{d\,(e'^2)}{d\,t}\left\{-\frac{3}{2}\,m^2 + \frac{3771}{64}\,m^4\right\},$$

which is the result obtained by M. Delaunay and myself.

The supplementary terms in the moon's coordinates which involve $\frac{d\,e'}{d\,t}$ are of the order of the disturbing force, and therefore the terms which they introduce into the integrals.

$$\int r^2\frac{d\,\text{R}}{d\,v}\,d\,v, \quad \int\frac{d\,\text{R}}{d\,v}\,d\,t, \quad \text{and} \quad \int d'\,\text{R},$$

will be the order of the square of the disturbing force.

This is the reason why $\frac{d\,\mathrm{h}}{\mathrm{h}\,d\,t}$, $\frac{d\,h}{h\,d\,t}$, and $\frac{d\,a}{a\,d\,t}$ are all of the order m^4.

It may be well to mention, in order to prevent any misapprehension, that in my memoir of 1853, h has not the same signification as the h of M. Plana's theory.

It is proved in Art. 11 of the Memoir that

$$2\int r^2 \frac{d\,\mathrm{R}}{d\,v}\,d\,v$$

contains the non-periodic terms

$$\mathrm{h}^2\left\{-\frac{285}{8}m^4 e'^2+\frac{495}{64}m^4 e'^2\right\},$$

$$=\mathrm{h}^2\left\{-\frac{1785}{64}m^4 e'^2\right\}$$

and the h^2 employed in the memoir is the absolutely constant quantity added to complete the integral, so that if for the sake of distinction $h_0{}^2$ be written for the h^2 of the memoir, we shall have

$$\mathrm{h}^2=h_0{}^2+\mathrm{h}^2\left\{-\frac{1785}{64}m^4 e'^2\right\}$$

$$\text{or }\ \mathrm{h}^2=h_0{}^2\left\{1-\frac{1785}{64}m^4 e'^2\right\}$$

The following relation exists between the h of M. Plana and the h of M. de Pontécoulant:—

$$\frac{\mathrm{h}}{h}=1+\frac{75}{64}m^4+e'^2\left[\frac{1125}{128}m^4\right].$$

Now this relation at once shows that if e' be variable, h and h cannot both be constant; and since no à-priori reason can be given why one of these quantities should be constant rather than the other, we are not justified in assuming that either of them is so.

This argument, however, does not appear convincing to M. de Pontécoulant.

In the two methods which, as I mentioned before, I employed previously to the publication of my Memoir of 1853, the value of $\frac{d\,n}{n\,d\,t}$ was deduced from those of $\frac{d\,\mathrm{h}}{\mathrm{h}\,d\,t}$ and $\frac{d\,h}{h\,d\,t}$ respectively. In the method which I now employ, $\frac{d\,n}{n\,d\,t}$ is determined by direct substitution in the differential equations, without introducing either the quantity h or h, that is, without taking into consideration the mean areal velocity at all.

In M. Plana's Memoir, contained in the eighteenth volume of the Turin Memoirs, he no longer maintains the constancy of his quantity h, but he determines its variation incorrectly, only taking into account part of the terms which produce this variation. M. Plana here recognises the reality of the supplementary terms involving $\frac{d e'}{d t}$, which I have proved to exist in the expressions for the moon's co-ordinates; and he finds values for δu and $\delta n t$ in pp. 14 and 20 of the Memoir, which coincide with mine, except in the terms with the argument $c' m \nu$, in which a mistake occurs in his coefficients, which, however, does not affect the coefficient of m^4 in the expression for the secular acceleration. It is very remarkable, however, that although he finds these values of δu and $\delta n t$, he does not substitute them in his equations, but puts $\delta u = 0$ and $\delta n t = 0$ instead of them. It is only by this strange process of suppressing part of the results which he himself has found, that M. Plana arrives at a different value of the secular acceleration from mine. Indeed, in the first form of this Memoir, as I have already mentioned, M. Plana did actually obtain a value coincident with mine.

M. Plana is led to make this suppression of his own results by a supposed à-priori proof that a certain integral which is equivalent to $2 \int r^2 \frac{d \mathrm{R}}{d \nu} d \nu$ can contain no such terms as those which would arise from the substitution in it of the true values of δu and $\delta n t$. Now, even if this proof had been ever so convincing, M. Plana was surely bound to show in what manner the terms thus arising from δu and $\delta n t$ were destroyed, as the different parts of his investigation would otherwise contradict each other.

In fact, however, this proof is entirely fallacious, for it rests on the assumption made at the top of p. 43 of the Memoir, that the terms multiplied by p, p^2, &c., in the equation given on the preceding page, may be neglected; and these are precisely the terms which are equivalent to those which M. Plana suppresses.

It may be as well to make another remark on this part of the investigation. In p. 42, M. Plana puts

$$e'^{g} \cos g \tau = \Sigma \mathrm{M} \cos (p \nu + q),$$
$$e'^{g} \sin g \tau = \Sigma \mathrm{M} \sin (p \nu + q),$$

and he assumes that all the coefficients p will be small quantities. But this will not be the case when $e'^{g} \cos g \tau$ and $e'^{g} \sin g \tau$ are thus expressed in terms of the moon's longitude. If these functions were similarly expressed in terms of the time, viz., if we were to put

$$e'^g \cos g\tau = \Sigma \mathrm{M} \cos(pt + q),$$
$$e'^g \sin g\tau = \Sigma \mathrm{M} \sin(pt + q),$$

all the coefficients p would be small.

The result which M. Plana obtains in this Memoir is

$$\frac{dn}{n\,dt} = \frac{d(e'^2)}{dt}\left\{-\frac{3}{2}m^2 + \frac{351}{64}\right\}m^4,$$

and the difference between this result and mine arises in the way I have explained, viz., from his having neglected to take into account the term

$$\mathrm{h}^2\left\{-\frac{285}{8}m^4 e'^2\right\}$$

which is shown in Art. 11 of my Memoir to constitute part of the non-periodic term of $2\int r^2 \frac{d\mathrm{R}}{dv}\,dv$.

M. Hansen's value of the secular acceleration is not exhibited in an analytical form, like those of MM. Plana and de Pontécoulant, and we can therefore only compare his numerical result with theirs. These differ considerably, and, in fact, much more than appears at first sight, on account of a reason which I will explain.

If we put

$$\frac{dn}{n\,dt} = \mathrm{K}\frac{d(e'^2)}{dt},$$

where K is the coefficient found from theory, the secular equation to be applied to the mean longitude will be

$$\mathrm{K}\int(e'^2 - \mathrm{E}'^2)\,n\,dt,$$

E′ being the excentricity of the earth's orbit at the epoch from which t is reckoned.

Now I find that M. Hansen uses a smaller value of the integral $\int(e'^2 - \mathrm{E}'^2)\,n\,dt$ than M. Plana does; that is, he supposes a slower change in the excentricity of the earth's orbit: and yet his resulting value of the secular equation is larger than those of M. Plana.

It may be inferred, either from the data in the Introduction to M. Hansen's Solar Tables, or from other data in the Introduction to his Lunar Tables, that the value of the integral

$$\int(e'^2 - \mathrm{E}'^2)\,n\,dt$$

which he employs is $-1212''\cdot5\,t^2$, t being expressed, as usual, in centuries.

Now M. Plana, in his Theory of the Moon, supposes the value of the above integral to be $-1264''{\cdot}1\ t^2$, and in his Memoir in vol. xviii. of the Turin Memoirs he gives it the value $-1297''{\cdot}7\ t^2$.

If, then, we reduce the coefficients of the secular equation given by these authors, so as to make them correspond with the value $-1270''\ t^2$ of the above integral, which is that employed in my Memoir of 1853, they will become

Coefficient according to M. Plana's theory	$10''{\cdot}60$,
,, ,, M. Plana's memoir (1856)	$11{\cdot}24$,
,, ,, M. Hansen's theory	$12{\cdot}76$.

The difference between M. Hansen's coefficient and either of M. Plana's is much greater than could possibly have arisen if both values had been found on correct principles, and they had differed merely in consequence of the approximations not being carried far enough.

My value of the same coefficient, which was communicated to the French Institute in January, 1859, is $5''{\cdot}70$. And M. Delaunay, while perfectly agreeing with me in the terms which I have calculated, has added a great number of others depending on the excentricity and inclination of the moon's orbit, and thus increases the coefficient to $6''{\cdot}11$.

As M. Hansen's method of obtaining his coefficient has not yet appeared, it is, of course, impossible for me to point out the reason of the difference between it and my own, as I have done in reference to the results of MM. Plana and de Pontécoulant. I have very little doubt, however, that it arises from M. Hansen having tacitly assumed, like M. Plana, that one of his constants introduced by integration is an absolutely constant quantity.

M. Hansen has suggested that the difference between his result and that obtained by M. Delaunay and myself may arise from want of convergency in the series proceeding according to powers of m, by means of which we determine the coefficient denoted above by K.

If we confine our attention to the terms of K which are independent of the excentricity and inclination of the moon's orbit, and which are admitted by all to constitute by far the largest part of that quantity, we find that the terms involving the successive powers of m taken into account by me give rise to the following parts of the coefficient of the secular equation:—

m^2...............	$10''{\cdot}66$,
m^4...............	$-2{\cdot}34$,
m^5...............	$-1{\cdot}58$,
m^6...............	$-0{\cdot}71$,
m^7...............	$-0{\cdot}25$.

The sum of these is $5''\cdot78$. The convergence, although slow at starting, becomes more rapid in the later terms; and I inferred, in my communication to the French Institute above mentioned, that the remainder of the series would be very nearly equal to $-0''\cdot08$.

Now M. Delaunay has since calculated the next term of the series, and finds it $=-0''\cdot06$, which is in exact accordance with my anticipations.

Although I think that there can remain no doubt with respect to the convergency of the series, yet, in order to remove all possible objection, I have calculated the value of K by a method which does not require any expansion in powers of m, and the resulting coefficient of the secular equation is

$$5''\cdot70,$$

exactly agreeing with that found by means of the series of powers of m.

A very few words will now suffice in reply to the objections which M. de Pontécoulant brings forward in his observations in the Monthly Notices. In fact, almost all of them have been virtually answered in what I have said before.

At the outset of his paper, M. de Pontécoulant rightly describes the difference between my method of finding the secular acceleration and all preceding ones, as arising from the consideration of the variability of the excentricity of the earth's orbit in the differential equations of the moon's motion, in which this element had hitherto been considered as constant. He then refers to the statement in my Memoir, that when this consideration was introduced into the formulæ, I found exactly the same result whether the time or the moon's longitude was taken as the independent variable. But, adds M. de Pontécoulant, "il n'y a qu'une petite difficulté dans cette assertion, c'est quelle énonce un fait mathématiquement *inadmissible*."

Now I confess that I cannot see M. de Pontécoulant's "petite difficulté." I am far from looking upon the agreement between the results of different methods as a fact mathematically inadmissible. On the contrary, it appears to me a palpable absurdity to suppose that the result of a mathematical investigation can be different according as one independent variable or another is employed in obtaining it, or that two methods of solving the same problem may both be correct and yet lead to contradictory results.

In order, however, to show this mathematical inadmissibility, M. de Pontécoulant goes on to say, "En effet, M. Adams convient quelque part, je crois, et d'ailleurs je le démontrerais bientôt jusqu'à l'évidence, que la considération de la variabilité de l'orbe terrestre, n'exerce aucune influence sur la détermination de l'inégalité séculaire, lorsqu'on emploie pour l'obtenir les formules directes que j'ai adoptées dans ma théorie."

In thus stating that I admit that one of the methods of determining the secular acceleration is unaffected by the consideration of the variability of the excentricity of the earth's orbit, M. de Pontécoulant overlooks "une petite difficulté," viz., that instead of admitting this, I assert, in so many words, the exact contrary. In the concluding sentence of my memoir I say, "when both processes were corrected by taking into account the new terms whose existence I had already recognised, I had the satisfaction of finding a perfect agreement between the results."

For M. de Pontécoulant's demonstration "jusqu' à l'évidence," I am not responsible, and indeed, I think his paper tends to show that he has peculiar ideas as to what constitutes demonstration.

In the next place M. de Pontécoulant offers "une réflexion très simple," which he thinks ought to have struck me. "Qui est-ce après tout que le coefficient de l'équation séculaire?—une certaine fonction des élements des orbites de l'astre troublé et de l'astre perturbateur, qui se déduit des formules différentielles du mouvement; cette fonction est la même, selon M. Adams, par quelque méthode qu'on l'obtienne, dans le cas ou l'on considère comme variable l'excentricité de l'orbe terrestre; à plus forte raison elle doit l'être dans le cas où l'on régarde cette excentricité comme constante." I am at a loss to imagine what can be the meaning of this last clause, since the secular equation in question is entirely due to the variability of the excentricity of the earth's orbit, and would not exist at all if this excentricity were constant.

It must be admitted that my new determination of the secular acceleration has, as M. de Pontécoulant says, "l'inconvénient d'altérer profondément l'expression analytique admise jusqu'à présent, du coefficient de cette équation," but truth must not be sacrificed to convenience.

In the algebraical portion of his paper, M. de Pontécoulant is not happier than in his introductory remarks. Indeed, throughout the paper he expressly leaves out of consideration all the terms which give rise to the difference between M. Plana's result and mine.

Thus, at the bottom of p. 311, having found from an assumed term in $\frac{dR}{dv}$, that

$$\int \frac{dR}{dv}\,dt = -\frac{A}{f}\,e'\cos(ft+l) + \frac{A}{f}\,\frac{de'}{dt}\sin(ft+l),$$

he incorporates the term involving $\frac{de'}{dt}$ with the preceding under the form

$$-\frac{A}{f}\,e'\cos\left(ft + l + \frac{de'}{e'\,dt}\right),$$

and then remarks:—

"On voit donc que la considération de la variation de l'excentricité de l'orbite terrestre ne fait qu' altérer d'une manière insensible la partie constante des angles des diverses inégalités lunaires multipliées par e', elle ne change en rien la forme des séries qui déterminent les co-ordonnées du mouvement troublé . . ."

Now these alterations of the constant part of the angles on which the several lunar inequalities depend, which are neglected as insensible by M. de Pontécoulant, actually give rise to the terms in the moon's co-ordinates involving $\frac{de'}{dt}$, which I have been the first to take into account, and thus do change the form of the expressions for those co-ordinates.

The term $\frac{A}{f}\frac{de'}{dt}\sin(ft+l)$ is not destroyed by being incorporated with the preceding term $-\frac{A}{f}e'\cos(ft+l)$, as M. de Pontécoulant seems to suppose.

Again, in order to show that the integral $\int\frac{dR}{d\nu}\,dt$ can contain no non-periodic term depending on e', M. de Pontécoulant assumes, at the foot of p. 310, that $\frac{dR}{d\nu}$ is made up of terms of the form

$$Ae'\sin(ft+l).$$

But $\frac{dR}{d\nu}$ is a function of r and ν; and since these quantities contain terms depending on the disturbing force and multiplied by $\frac{de'}{dt}$, $\frac{dR}{d\nu}$ will contain, in addition to the terms of the form considered by M. de Pontécoulant, other terms of the order of the square of the disturbing force, and of the form

$$B\frac{de'}{dt}\cos(ft+l);$$

among these there will be a term in which the angle $ft+l$ vanishes; viz., one of the form

$$Ce'\frac{de'}{dt},$$

and consequently $\int\frac{dR}{d\nu}\,dt$ will contain the non-periodic term $\frac{1}{2}Ce'^2$.

M. de Pontécoulant characterises the process which I have employed at the bottom of p. 402 in my Memoir, in order to find the non-periodic parts of certain integrals, as "une véritable *supercherie analytique*." Now this "supercherie" only consists in taking account of the variability of $\frac{de'}{d\nu}$, by putting for it the identical quantity $\frac{de'}{dt}\cdot\frac{dt}{d\nu}$.

M. Plana, in equation [10], p. 12, of his Memoir, finds, for the terms thus objected to by M. de Pontécoulant, exactly the same values as I have done, though his process entirely differs from mine.

On this same point, in a note to p. 315, M. de Pontécoulant makes the objection that in the last step of the integrations referred to I make $d\nu = n\,dt$, contrary to the supposition I had previously employed. But my object was simply to find the non-periodic parts of the integrals concerned; and it is obvious that if I had put for $d\nu$ its complete value $n\,dt - \phi(\nu)\,d\nu$, where $\phi(\nu)$ is a periodic function of ν, this function would only introduce periodic terms into the integrals, and would cause no change whatever in the terms which I have found.

But one of the most remarkable objections in the whole course of M. de Pontécoulant's communication occurs in p. 316, where he says he is going to put his finger on the error I have committed. From an equation in my Memoir he deduces the following:—

$$e' = q + q'\left\{\nu - \frac{11}{8}m^2 \sin(2\nu - 2m\nu) - \frac{77}{16}m^2 e' \sin^2(2\nu - 2m\nu - c'm\nu) + \&c.\right\}$$

and then adds the remark,—

"C'est à dire, que l'excentricité de l'orbite terrestre, outre sa variation séculaire, serait soumise à toutes les inégalités du mouvement lunaire; c'est à dire, à des variations dont le période serait d'un mois, d'une année, &c. ce qui est contraire, quelque petitesse qu'on suppose au coefficient q', à tous les principes de la théorie."

Now it is astonishing that M. de Pontécoulant does not see that the quantity enclosed within brackets, in the above equation, is simply the expression of the moon's mean longitude nt in terms of the true longitude ν, so that the equation is equivalent to

$$e' = q + q'nt;$$

that is, the excentricity of the earth's orbit is made to vary uniformly with the time, which agrees with the supposition with which we started.

On the other hand, M. de Pontécoulant, by making

$$e' = q + q'\nu,$$

that is, by supposing the change in e' to be proportional to the moon's true motion in longitude, would evidently cause the excentricity of the earth's orbit to be affected by all the inequalities of the lunar motion.

All attempts to express e' in terms of ν, without introducing periodic terms, lead to this absurdity.

I have already alluded to the strange notion expressed at the end of M. de Pontécoulant's paper, that there may be two values of the secular acceleration, one applicable to the true longitude and the other to the mean longitude. The difference between the true and the mean longitudes consists wholly of periodic quantities, and cannot contain any term increasing continually with the time.

How M. de Pontécoulant could have so far deceived himself as to imagine that this paper settled the question of the secular acceleration, "sans contestation possible désormais," is, I confess, beyond my comprehension.

P.S.—In the *Compte Rendu* of April 9, 1860, which has appeared since the foregoing paper was read, M. de Pontécoulant gives the value of the secular acceleration of the moon's mean motion, which he has obtained by taking the time as the independent variable, and which he considers to be "désormais à l'abri de toute objection."

This result, however, of M. de Pontécoulant's is the same as that which he formerly communicated to me, the error of which I have already pointed out.

M. de Pontécoulant thus describes his method, "En développant la formule qui donne l'expression de la longitude vraie en fonction de la longitude moyenne, et en n'ayant égard qu'au premier terme de ce développement, c'est-à-dire à sa partie non-périodique j'en ai conclu le rapport du moyen mouvement de la lune dans son orbite troublée au moyen mouvement relatif à son orbite elliptique, c'est-à-dire à l'orbite que cet astre décrirait autour de la terre sans l'action du soleil. . . . En différentiant ensuite cette valeur par rapport à l'excentricité e' de l'orbite terrestre qu'elle renferme, . . . j'ai obtenu une expression de cette forme:

$$\frac{\delta n}{n} = \mathrm{H}\,\delta\,.\,e'^2."$$

The value of H thus obtained is

$$\mathrm{H} = -\frac{3}{2}m^2 + \frac{5337}{128}m^4$$

which, as I have shown in p. 9, is the result that would be found by differentiating the relation between n and a, and then neglecting the variation of a. The fallacy of M. de Pontécoulant's reasoning consists in his treating the moon's "orbite elliptique, c'est-à-dire, l'orbite que cet astre décrirait autour de la terre sans l'action du soleil," as if it were a real elliptic orbit with an unalterable semi-axis major, whereas the semi-axis major of the elliptic orbit spoken of by M. de Pontécoulant, which is the same quantity as that above denoted by the symbol a, is really variable, and its variation must be found by means of the differential equations in the way which I have before described.

The numerical value of the coefficient of the secular equation which M. de Pontécoulant obtains in this paper, when reduced so as to correspond with the value $-1270'' t^2$ of the integral $\int (e'^2 - E'^2)\, n\, d t$ is

$$7''\cdot 96$$

which, as we see, differs widely from the similarly reduced values of the coefficient according to the theories of M. Plana and M. Hansen, given in p. 14, as well as from the values obtained by M. Delaunay and myself.

After giving his formula for the secular equation, M. de Pontécoulant remarks, "En comparant ce résultat à celui que M. Plana a déduit de ses formules, on voit qu'il en diffère d'une manière notable, et que l'espèce de compensation qui devait s'établir, selon ce géomètre, entre les quantités du quatrième ordre et celles des ordres supérieurs, et qui semblait permettre de s'en tenir, comme l'avait fait Laplace, aux termes résultans de la première approximation, n'existe pas réellement. La considération des puissances supérieures de la force perturbatrice altère sensiblement, au contraire, la valeur du coefficient qu'on obtient en faisant abstraction des quantités qui en dépendent, et comme tous les termes de la formule, jusqu'aux termes du septième ordre, sont affectés d'un signe négatif, la grandeur du coefficient qu'on s'était habitué à supposer à l'équation séculaire d'après les indications de Laplace, doit être considérablement diminuée."

It is needless for me to point out how totally inconsistent these remarks of M. de Pontécoulant are with the conclusion at which he arrives in his paper in the *Monthly Notices*, "Il résulte, je pense, sans *contestation possible désormais*, de la discussion précédente, qui les formules employées jusqu'ici pour déterminer l'équation séculaire de la lune, ont toute la correction nécessaire à cet important objet."